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The observation of December 18 was near the maximum epoch No. 567, reckoning from an assumed maximum observed by me 1876 December 14.

Some Experiments relating to the Method of Obtaining the Coefficient of Absorption of the Wedge Photometer. By Edmund J. Spitta.

In a paper read before the Society in November 1887, on ‘The Appearances presented by the Satellites of *Jupiter* during transit,"* attention was called to the discordant figures obtained when photometrically comparing a point of light, such as, for example, is exhibited by the stars, with an object of sensible area such as *Jupiter*. For the past two years or more an extensive series of experiments with the wedge photometer has been in progress, with the object of ascertaining the cause or causes of such discordance; but the subject has extended itself so much further than was anticipated, that it will be convenient to describe the results under three heads: (i) Those connected with the constant of absorption of the wedge photometer; (ii) the objections to employing such a form of photometer when dealing with objects of sensible area and the means devised to remedy the same; and (iii) idiosyncrasies in the photoperceptive faculty of the eye when viewing different sized areas. The object of this communication is to set forth the experiments connected with the first division of the subject only—viz., those concerning the constant of absorption of the wedge photometer, with a suggestion for an improved method of obtaining the same.

In the course of some experiments made at an early stage of the inquiry, with light of varying but known intensity, a suspicion was raised that the results obtained by the method of wedge extinction were incorrect. This suspicion was entertained both with experiments made on points of light, and areas of equal diameters but of different degrees of illumination.

The readiest means for the complete examination of this question was evidently to conduct the experiments in such a manner as to ensure the addition of a known quantity of light to the point or area under examination. In my earliest experiments I arranged three gas burners in an equilateral triangle, any or all of which could be made to illuminate a small disc of cardboard equidistant from the three, and similarly situated with regard to the gas burners. The wedge was used without a telescope, though a telescope tube was found convenient both to carry the wedge and give the means of easy direction. The intensity of the illumination of the cardboard disc by each of the gas burners was first examined and made sensibly equal, and then the intensity of the illumination when increased by the

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* *Monthly Notices*, vol. xlviii.

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addition of each of the gas jets in turn was carefully determined by the method of wedge extinctions. Although it seemed certain from the precautions taken that when the combined light of the three gas jets was allowed to fall on the cardboard disc that this disc was illuminated by a light three times that which it exhibited when illuminated by only one, yet the indications of the photometer showed that the light of the disc was that which would be given by three and a half burners.

A repetition of the experiment under slightly different circumstances, and made with another apparatus, constructed, if possible, with still greater care, showed the same amount of error, and in the same direction. It was therefore determined to conduct the experiments on a larger scale—*i.e.*, with greater range of illuminating power, in which the errors of observation could not have so large a proportion to the final result. With this view, an argand burner was firmly fixed on a solid horizontal board provided with cross pieces to prevent warping. Around it at equal distances, but on another board placed at right angles to the first, was arranged a series of twelve 1-inch mirrors, each being provided with a ball-and-socket-joint to allow of the necessary accuracy of adjustment in the subsequent equalisation of the lights. The reflected light from every mirror passed through a separate hole in another board placed between the lamp and the disc, and every aperture was provided with an accurately fitting wooden shutter. The mirrors were entirely enclosed during the final measurements, the room being carefully darkened. The preliminary adjustment of the mirrors led to the rejection of four, because they reflected a smaller amount of light than the others, due to their position relatively to the lamp. One of the mirrors having been arbitrarily selected as the standard, the light from each of the others was made equal to it within the errors of observation. This will be seen from the following table, which gives the intensity of the illumination of the cardboard disc when the light from each of the mirrors was allowed to fall on it in succession. Calling the light—nominally converted for convenience into magnitude—of the standard mirror (1) L, that of each of the others proved to be:—

TABLE I.

No.	Mag.	No.	Mag.
2	L−·042	6	L−·014
3	L−·092	7	L+·092
4	L−·075	8	L+·014
5	L+·075		

Before quitting the subject of adjustment, it may be remarked that, whatever difference may have previously existed in the reflective power of the mirrors, whether it arose from the silvering or from their position with respect to the lamp, is of no moment,

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seeing that their light as reflected from the disc and viewed in the wedge photometer had been made as nearly equal as possible; and further, the light of the lamp is eliminated. All that is necessary was to ascertain that the light of the lamp did not vary during each series of observations, and this was readily effected by watching the light of the disc when illuminated by the mirror chosen as the standard

The experimental measures were then commenced, the mirrors being numbered from one to eight. The mean extinction of the standard was first obtained, shutter two then opened and the increase of light measured, shutter three next, and so on to the end of the series. On a subsequent night the whole of the observations were repeated, but this time the conjoint illumination from the first and eighth mirrors were compared with the standard to obtain the increase of light reflected by two mirrors, adding the value of the seventh to arrive at the increase afforded by three and so on, by which means it was thought that the possibly slight inequality of the illuminating power of the mirrors would be eliminated, the small discrepancies shown in Table 2 being attributed to the unavoidable errors of observation which they do not exceed.

Table II.

No.			
2	·76	·075	2·01
3	1·33	·05	3·40
4	1·85	·10	5·50
5	2·21	·04	7·66
6	2·44	·045	9·46
7	2·77	·08	12·83
8	3·03	·025	16·30

In column 1 is shown the number of mirrors employed, in other words the intensity which the wedge photometer must exhibit, if properly evaluated. Column 2 the difference in magnitude actually obtained by the wedge. Column 3, the probable error of this in magnitude, and column 4 the successive increase in light derived from the magnitude given in column 2.

It may be here stated that these figures were not hastily accepted, in fact not until every possible source of error that could be thought of, such as the effect of fatigue of the eye, sudden alterations in the pressure of the gas, the effect of using large or small discs, had been eliminated.

Two remarks, however, are naturally suggested by a consideration of this table. First that the increase in the amount of light as shown in column 4 does not appear to take place with the regularity which the addition of equal increments of light seems to demand But this difference is in a sense more apparent than real, arising from the necessary employment of a logarithm to convert the observed magnitudes into light-ratio. The point which is insisted on is that every addition of a unit of

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light is accompanied by an observed result greater than the unit. The second remark is this; that apparently, and as far as the table is concerned, a factor for the conversion of magnitude into light-ratio other than the conventional ·4 might be chosen which would satisfy the condition observed. Practically, if ·3 were taken, or if it were assumed that the light of each magnitude were double that of the one preceding, a tolerable agreement might be effected; but this is impossible because in the determination of the constant of absorption of the wedge, the factor (·4) has been employed, owing to the original determinations having been made in light, and the differences in magnitude deduced by the convention.

A satisfactory explanation of the result arrived at in Table II. is not easy. The most plausible was that some error had been made in the evaluation of the wedge, or else that its absorbent properties had undergone some change after the numerous extinctions with which it had been employed in the *Jupiter* experiments. Accordingly resort was made to the method of evaluation as adopted by Professor Pritchard and explained by him in his Memoir on the wedge photometer,* and which is dependent upon the physical nature of polarised light. After acquiring a reasonable familiarity with the polarising apparatus, a determination of the coefficient of absorption over different portions of the wedge was effected and the result compared with that obtained by Mr. Plummer, who had kindly made the one used in the results given in Table II. It was found the two determinations were almost identical, the discordance not exceeding ·06 magnitude. This at once set aside the question of possible error in the evaluation, and proved that the wedge had not undergone any change from age and use. The only point to which it seemed that objection might yet be made in these experiments, was the employment of a disc which of necessity occupied some area on the wedge when undergoing extinction. The use of a point of light would vary the conditions somewhat under which the experiments had been made, and more nearly resemble the observations made on stars. By using an artificial star, which was effected by illuminating one of the small holes mentioned in the *Jupiter* experiments,† and controlling the light issuing through it by an Abney photometer, the following series of measures was obtained:—

Table III.

No.			
2	·92	·007	2·33
3	1 34	·035	3·43
4	1·63	·063	4·48
5	2 26	·021	8·05
6	2·36	·085	8 80
7	2·81	·028	13·31
8	3·03	·016	16·30

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* *Memoirs R.A.S.*, vol. xlvii.

† *Monthly Notices*, vol. xlviii., and *The Observatory*, vol. x.

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The arrangement of the figures in this table is precisely the same as in Table II.; and in estimating its accuracy or its apparent deviations from uniformity the remarks made on that table must be rigidly borne in mind. As a general rule, it will be seen that the resulting magnitudes of column 2 do not differ in the two tables by quantities much greater than those shown in the probable errors. The observations were thought to be easier for the brighter lights, or at least left a more favourable impression in the observer's mind, and in the case of the full brilliancy of eight lights the coincidence is exact. The interest is not lessened by the consideration of the fact that in the first set, reflections off a disc of sensible area were employed, but in the second, the augmentation in intensity was effected in a point of light.

At this juncture an opportunity occurred of testing another wedge by the polarising method, one made precisely after the model of that used by Professor Pritchard. The constant of absorption, carefully determined, gave a ratio of 1 :16·40 instead of the real ratio of 1 : 8 which strikingly corresponded with the 1 : 16·30 obtained with the Clapham wedge, which was of a shorter and consequently steeper make, or one where a denser pigment had been employed in its manufacture.

As no solution of the difficulty presented itself, suspicion was attached to the accuracy of the sine law of Malus,* which underlies the polarising method adopted by Professor Pritchard, or rather, admitting the theoretical accuracy of the law, it did not seem impossible that the practical application of it might be attended with difficulties, and an unsuspected systematic error vitiate the results arrived at by its use. I proposed accordingly to test the practical application of the law by means of the Abney photometer; but the matter led to a protracted series of experiments, their entire discussion forming the subject of a paper communicated to the Royal Society by the kindness of Captain Abney, and read in June 1889, from which the substance of what follows is extracted. Before, however, giving a brief account of the experiments themselves, the precautions taken with them, and the results derived therefrom, it is necessary to hastily state the principle of the process employed by Professor Pritchard. If a Nicol's prism, capable of turning freely about its axis, be attached to a double-image prism, and two illuminated apertures (narrow rectangular slits are employed by Professor Pritchard) be viewed through this apparatus, there will be seen, as is well known, owing to the employment of the double-image prism, two images of each aperture, which can be successively made, by turning the Nicol through a definite angle, fainter and fainter till final disappearance of one pair of images supervenes. If the Nicol be furnished with the means of recording the angle through which it is turned, and (q) be the reading for the dis-

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* Paris Académie des Sciences. *Mémoires présentées par divers Savants*, tome 2, 1811.

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appearance of one of the images, and (q') be the reading for another position of the Nicol, then the brilliancy of the object (P) will be expressed by $\mathrm{P}\sin^2(q'-q)$, and $\mathrm{P}\cos^2(q'-q)$. Similarly, the brilliancy of the second object or aperture Q will be expressed by $\mathrm{Q}\sin^2(q'-q)$, and $\mathrm{Q}\cos^2(q'-q)$. Whence, making the images derived from the *extraordinary* ray of P equal to that produced by the *ordinary* ray of Q, the ratio of the light of P to Q will be expressed by

$$\tan^2(q'-q).$$

Practically it is convenient, owing to the difficulty in determining the precise angle at which the extinction of either ray occurs, to observe equalities of brilliancy on each side of the zero, and this is the method which Professor Pritchard has adopted for determining the absorptive power of a medium, by placing a certain portion of his wedge behind the rectangular apertures already mentioned, and measuring by the method indicated the ratios of the light transmitted through those two portions, the distance between which is carefully measured. It was originally intended to employ in these experiments the method just described in evaluating the constant of absorption of the wedge, but an objection presented itself, for the two images not under examination were never of equal brilliancy whilst the observation for equality in the other two was being effected. If small differences of intensity were being compared (as for instance in evaluating the wedge) this was of no consequence, but when illuminations of greatly different intensity were employed, the disturbing effect was so distracting, that it was felt grave doubts as to the accuracy of the observations might be justly entertained. It was determined therefore to modify the arrangement by using a single slit, Captain Abney's photometer being so arranged as to diminish at will the illumination of either ray. The figures obtained varied considerably, the observations being characterised by a large observational error, but the ratio indicated by the polarising apparatus *was always greater* than that shown by the Abney photometer. Much larger prisms were then obtained, and a more perfect apparatus constructed, the images being focussed by a lens across a portion of the observatory on a screen, the observer being surrounded by an improvised wall to secure darkness during the subsequent equalisation of the lights. Still the same result was manifest, the ratios exhibiting the same peculiarity as in the earlier experiment, although be it noticed the observational errors were much smaller. The assistance of a friend well accustomed professionally to the equalisation of lights was then sought, but the independent measures by him again supported the previous experience. To find out the cause or causes of this discrepancy between the ratios obtained and those known to exist, became the subject of a most protracted inquiry, but after eliminating all possible causes of error in the apparatus, such as might occur from irregular illumination of

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the slit, deviation in position of the lamp employed from night to night, eccentricity of the Nicol, different absorptive power of the two halves of the double-image prism employed, condition of the lens used to focus the images on the screen, positions of the prisms with respect to the line of collimation, and the possible fluorescence of the material employed in the manufacture of the screen, a suggestion due to Captain Abney, it was ultimately found to be due to *polarised reflections from the inner surface of the Nicol's prism*: rays which were found to utterly prevent the correct equalisation of the images, either by augmenting the angular reading in one position of the Nicol or detracting from it in the other. After some time, the effect of these reflections was discovered to be completely eliminated by the use of a diaphragm, placed between the eye and the Nicol, for after the employment of such, the ratios came out reasonably accordant with those known to exist, as the following table, extracted from the paper in question, fully shows:—

TABLE IV.

Known ratios.	Obtained by experiment.
1 to 2	1 to 2·14
3	3·02
4	3·84
5	4·97
6	5·80
12	11·97
18	17·11*

The crucial test, however, of the truth of the assertion that the cause of the increase in the coefficient of absorption, as obtained by Professor Pritchard's method, lay in the fact that the polarised reflections from the internal surface of the Nicol were not cut off by a suitably adjusted diaphragm, yet remained to be proved. For this purpose the rays issuing from the Nicol having been focussed on the screen, and the precautions mentioned above having been most carefully introduced, a fresh evaluation was made of the wedge, when the resulting coefficient closely accorded with that obtained by both the previous apparatus, as the following table clearly exhibits:—

TABLE V.

	Mag.
By the method of reflection from mirrors	·596
By the employment of Abney's photometer	·588
By the polarising method WITH diaphragm	·580
(By the polarising method WITHOUT diaphragm	·71)

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* The cause of the falling off in this particular ratio is duly accounted for.

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It may seem to those who are not engaged in practical photometry that the employment or non-employment of a diaphragm is a very small matter, but that would be altogether to underrate its importance. My real object in communicating the results of these experiments to the Society is to point out the undesirability of employing a method of evaluating which without great precautions is likely to lead to uncertain results. Initially the method, as hitherto practised, is open to a further objection, that the difference of the intensities of light compared by means of the polarising apparatus *must be small*; that is to say, the rectangular slits must be near together. In Professor Pritchard's case, the distance between the slits was ·38 inch; and though no doubt his caution and experience enabled him to reduce the error of the determinations of the constant to a minimum, yet it is nevertheless the fact that if the difference of wedge reading be 3·8 inches, this error, however small, must be multiplied by ten. An advantage of the polarising method, which must not be lost sight of, is that it offers a very ready means of examining the *uniformity* of a wedge, far more so, indeed, than any other with which I am acquainted. If the intensities at any two places on the wedge be equalised by a certain position of the Nicol, then that same equalisation *must* be maintained for all other positions of the wedge, the light of both slits being equally increased or diminished as different portions of the wedge are presented to view. But after considerable experience I am convinced a more satisfactory and reliable plan to obtain the actual *constant of absorption* consists in regulating the intensity of a point of light by some such an arrangement as the photometer devised by Captain Abney, and determining the point of extinction on the wedge. In this way not only is the instrument evaluated by the same method as it is used, viz., by a process of extinction, but intensities differing in the ratio of 1 : 32 (or nearly four magnitudes) may easily be obtained, and the accumulation of error referred to above effectually avoided.

I hope nothing I have said will be so misconstrued as to induce the belief that I have any hesitation in accepting the results derived by the aid of a wedge photometer. Far from it; I would call attention particularly to the fact that the observations made with such an instrument are *more* in accordance among themselves than is the case with those made with polarising apparatus. Whether, as I am inclined to believe, and have explained above, the effect of internal reflections, to some extent lawless but yet so significant, is to enter into photometric observations made with polarising apparatus (some positions of the Nicol requiring the *very greatest* care to ensure their effectual elimination), I must leave to be settled by those versed in the use of such instruments; but the difference between the results of Dr. Wolff and Professor Pickering * will be recalled by all. What I do suggest, however, is that there is the possibility of introducing

* *Annals of Harvard Observatory*, vol. xiv.

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a systematic error in observations made with a wedge where the error, arising from the coefficient of absorption, *is cumulative in its character*; but—and this, I submit, is a matter of great importance—this systematic error is of a character which can be removed, as I have pointed out, *not* by any comparison with observations of the same kind by other authorities, and the possible introduction thereby of a fresh source of error, but by a strictly independent and scientific investigation, with some form of photometer such as that devised by Captain Abney.

Ivy House, Clapham Common.

On a Method of Obtaining the Error of a Chronometer by Equal Altitudes of two Stars on Opposite Sides of Meridian. By A. Mostyn Field, Commander R.N.

The principle of this method depends upon the sidereal time of passing the meridian of a place, by an imaginary star, having the mean right ascension of the two stars selected, being compared with the time shown by a sidereal chronometer at that instant; the difference is its error on sidereal time. A mean solar chronometer can be used equally well. The sidereal time required is the mean of the right ascensions of the two selected stars. The chronometer time (either mean or sidereal) at that instant is the mean of the times at which the eastern and western stars had equal altitudes, with the "equation of equal altitudes" applied with its proper sign.

Equation of Equal Altitudes.

The rigorous expression, according to Chauvenet, is :—

$$\sin \alpha = \cot \tfrac{1}{2}\,\text{E.T.} \tan d \tan \delta \cos \alpha - \operatorname{cosec} \tfrac{1}{2}\,\text{E.T.} \tan l \tan \delta,$$

where

$\alpha =$ equation of equal altitudes $= \dfrac{h-h'}{2}$,

$\tfrac{1}{2}$ E.T. $= \tfrac{1}{2}$ elapsed time $= \dfrac{h+h'}{2}$.

$d =$ declination at upper meridian passage.

$d-\delta =$ declination at observation E. of meridian.

$d+\delta =$ declination at observation W. of meridian.

$\delta = \tfrac{1}{2}$ difference of the declinations at the two times of observation.

h and $h' =$ hour angles from noon at east and west observations respectively.

In the case of equal altitudes of two stars, one east and the other west of meridian, the above formula is strictly accurate whatever may be the difference in the declinations; the half elapsed time being found as follows :—